Letters to the Editor.

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The Resting-place of Robert Boyle.

SOME months ago, having become very much interested in the life and work of the famous English philosopher, the Hon. Robert Boyle, and animated to a certain extent by the spirit of the pilgrim, I thought I would go and look at his last resting-place. According to Thomas Birch, who brought out in five folio volumes the works of Boyle with a "Life" in the year 1744, the philosopher died on December 30, 1691, seven days after his sister Katherine, Lady Ranelagh, with whom he had been keeping house in Pall Mall for more than twenty years. Brother and sister were buried in the chancel of St. Martin's-in-the-Fields.

On reaching the church I learned that the old church in which they had been laid was pulled down in 1721 and replaced by the present edifice. On inquiry of the rector he referred me to his churchwarden, Mr. John MacMaster, from whom and from his interesting book on the church the following facts are derived :—

The foundation-stone of the new church was laid by the Bishop of Salisbury on March 19, 1721. James Gibbs, a pupil of Wren's, was the architect. "As the bodies buried in the church and part of the churchyard would be disturbed during the rebuilding, an advertisement was inserted in the newspapers notifying that the bodies and monuments of any of those buried could be taken away for reinterment by relatives on application to the Vicar, Wardens, and Commissioners. Several bodies and monuments were removed." It appears, however, that applications for permission to set up family monuments in the same position in the new church as in the old were not granted, and those monuments from the old church which were not taken away by relatives were stored under the tabernacle or, in some cases, set up in the vaults and crypt of the present church. It is on record that "Robert Boyle, the gifted son of the Earl of Burlington," was among those buried in the church, but no systematic account was kept of the disposal of the remains in the old church, and there is no monument bearing the name of Boyle in the crypt at the present day.

As it seemed possible that there might be some tradition in the family of action taken by them in 1721 to preserve the remains of the philosopher, I wrote to the present Earl of Cork and Orrery, but could get no information. Later, Lady Grace Baring (*née* Boyle) informed me that after looking into books of family records in her possession no clue could be found to the mystery of Robert Boyle.

It is remarkable that Birch's account of the funeral and burial should have been published without comment or correction in 1744, or more than twenty years after the destruction of the old church. No modern biographer seems to have inquired further into the matter, and it seems probable that the last resting-place of the "Father of Chemistry" will remain unknown to the end of time.

WILLIAM A. TILDEN.

September.

Biological Terminology.

DR. BATHER (NATURE, August 18, p. 778) wishes me to explain my glaring truism, "Variation is the sole cause of non-inheritance: apart from variations, like exactly begets like when parent and child develop under like conditions" [of nurture]. But does it need explaining? As he says, and as I have insisted, variation *is* non-inheritance, and for that reason the truism is glaring. The words "the sole cause of" are really redundant, and were introduced merely to emphasise the fact that there is no other cause. My justification for framing the truism lies in the fact that that truth is more honoured in breach than in observance in biological discussions. I have already expressed myself much in the following terms, but some repetition seems necessary. Every character is a product of antecedent *and* exciting cause, of nature *and* nurture, of potentiality *and* stimulus, of power to develop *and* opportunity to develop. Since the multicellular individual is derived from a germ, he can inherit only through it. In the germ are none of the characters subsequently developed in the soma, but only powers to develop them. Therefore, strictly speaking, he inherits nothing but these powers, the sum of which is his nature, while the sum of the influences which cause change (or arrest it) is his nurture. By a colloquialism, which is pardonable since it confuses no one, we speak of a child inheriting his parent's eyes, or hair, and so on. If a child in response to similar nurture produces hair like his parent's, he has not varied in this respect; he has inherited; he is like his parent both by nature and through nurture. If he develops different hair in response to similar nurture, he has varied; to that extent he has not inherited. If owing merely to different nurture (*e.g.* injury) he produces different hair, or even none at all, he has *inherited,* but not *reproduced.*

Inheritance is altogether an affair of nature; reproduction implies the added element of nurture. Reproduction is proof of inheritance; but non-reproduction is not proof of non-inheritance. There is, indeed, massive evidence of inheritance without reproduction—*e.g.* in latent ancestral traits, male characters in the summer generation of aphides, and the recessive in the impure dominant. If for "hair" we substitute in the foregoing *any* of the characters which biologists call "acquired," and use our words with the same meanings, then all I have said remains exactly true. For example, if a parent and child receive similar injuries and develop similar scars, then the child *inherits* the scar. He would really have inherited even if he had not received the injury and developed the scar. But biologists no not give their words with the same meanings. If a child produces an "acquired character" in the same way as the parent did (if he is like the parent both by nature and through nurture), they say he has not inherited, but acquired, that trait afresh—as if every trait were not acquired afresh every time. They assume that he would "inherit" only if he reproduced the same trait in response to some different nurture, only if he did not inherit, only if he were unlike the parent both by nature and through nurture. The word "inherit" now means "vary." Now comes my point. The truism is founded on the assumption that all characters that can possibly be developed are, *necessarily,* and in exactly the same sense, equally innate, acquired, germinal, somatic, and inheritable. Nearly all biological discussions (*e.g.* the Neo-Lamarckian and Neo-Darwinian) are based on contrary assumptions, and imply, therefore, the denial of the truism. If it were accepted *and borne in mind,* most of the labours and disputes which have occupied biologists would automatically end, and the way be cleared for further advance. Is this the explanation that Dr. Bather desires?

Can Dr. Bather tell us in terms of utility why, when all characters are equally innate, acquired, and inheritable, biologists should distinguish them from one another by these terms? I am sure he will find the task impossible. Fortunately, the historical explanation is clear. Formerly it was universally believed (and still is popularly) that the soma of the child was derived, by means of physiological units, gemmules, or what not, from that of the parent—the child's head from the parent's head, and so on. Granting that assumption, this, the child's heritage, was innate. But it could be altered, as by injury and use, and these alterations were acquired. If the soma of the child took origin in that of the parent, it was reasonable to believe that characters acquired by the parent could be transmitted in some degree to the child in whom they would be innate. Hence the Lamarckian hypothesis. Obviously, with this conception of heredity, the application of the distinguishing terms "innate," "acquired," and "inheritable" was intelligible and correct. Characters actually were innate *or* acquired, inheritable *or* non-inheritable. Nature and nurture were warring forces, and attempts to estimate their relative strength were reasonable. But no clear distinction could be drawn, and, in fact, none was drawn, between variations and modifications, which were all called variations. The truism was not true, for what we now recognise as modifications altered the heritage, and like did not produce like under like conditions of nurture alone.

After the discovery of the cellular structure of the body the belief grew that the heritage travelled solely down the germ-tract. If that were true, only potentialities to develop in this or that way were transmissible; all characters were equally products of nature and nurture; the terms "innate," "acquired," and "inheritable" became inapplicable to characters as distinguishing terms; the physiological classification of characters as responses to various stimuli became necessary and clear, as did also the distinction between variations and modifications; nature and nurture were not warring, but co-operating, forces; the Lamarckian discussion became absurd, for valid reasoning cannot be founded on non-existent differences; the truism became true, and, since it covered almost the whole area under discussion, there ensued a vast simplification of study. One now thought of development in terms of nurture, and evolution and heredity in terms of the potentialities of the germ-plasm, which was conceived as changing through the ages, by the accumulation of variations in it, in such ways that some of the old potentialities were eliminated or altered and some new ones evolved. For example, one conceived man as differing from the oak because his nature, and therefore his nurture, was different. One ceased to regard him as a being compounded of innate and acquired characters.

Biologists failed to perceive fully the necessary implications of their own discoveries. "Men believe that their reason rules over words; but it is also the case that words react, and in their turn also use their influence on the intellect." The words "innate," "acquired," and "inherit," sanctified in usage by tradition, exercised a fatal influence. Instead of perceiving that all characters are equally innate, acquired, and inheritable, biologists proceeded to fortify the error by coining new synonyms ("germinal," "somatic," "blastogenic," and the like) and to discuss the "transmission of acquired characters," "the intensity of inheritance," and so on. In other words, the old notion that the parts of the child are derived from those of the parent was wonderfully combined, by means of a misuse of language, with the new and quite incompatible belief that the heritage travels down the germ-tract. When a supposition is true or untrue it may be proved or disproved. When it is nonsensical it can be neither proved nor disproved. Crucial testing is then impossible; discussion leads merely to more nonsense. At once the word "inherit" acquired, quite unnoticed, two contrary meanings, and, besides, was often used as synonymous with "reproduce." Endless controversy arose; sects developed; holy ways of gathering (*i.e.* restricting) evidence were acclaimed; the habit of calmly examining all the evidence by means of crucial testing fell very largely into abeyance. Thus did biology achieve the scientific status of theology and politics. Hence the present chaos. Hence the fact that, though biologists believe that all characters that develop in response to use are "acquired," they, following tradition, limit the term to some only of the characters that so develop. For example, while they term the muscles of the blacksmith "acquired," they consider those of the child and the ordinary man, which grow in response to similar nurture, "innate." Hence the failure to observe the magnitude of use-developments in the higher animals, and, therefore, to perceive what is for human beings the most important phase of all evolution, that of the power of so developing. Hence the fact that I, a mere outsider, neither zoologist nor botanist, am able to write so impudently without fear of being eaten like a shrimp, as would have happened had I assailed physics, chemistry, mathematics, astronomy, or descriptive zoology or botany.

Biologists may declare that their terms have technical meanings. But no such meanings which accord with usage can be thought of. The evidence in literature is plain that every writer has really believed that some characters are more innate, acquired, and inheritable than others. Witness the synonyms. Again, biologists may declare that when they discuss Lamarckian "inheritance" they merely consider whether these so-called acquirements so alter the germ-plasm (cause it to vary) that a character, which evolution fitted ancestors to develop in response to use or injury, tends to be developed by descendants in some other way—*e.g.* in response to a hormone. But why not use plain and correct language? Why designate characters which are not especially innate, acquired, and inheritable by those terms? Why say "inherit" when exactly the opposite (vary, non-inherit) is meant? Why suppose that characters which develop in response to use or injury (and only some of these) have this amazing property of causing such wonderful germinal alterations? Why not speculate as to whether the development in ancestors of ordinary muscles in response to use tends so to change the germ-plasm that ordinary muscles can be developed in descendants in response to other influences—*e.g.* hormones? Why not question whether the development of hair and teeth in ancestors in response to hormones tends to cause their development in descendants in response to other influences—*e.g.* use or injury? Use, injury, hormones, and the rest of what we call nurture are all influences to which evolution has made the individuals of the different races responsive; why, then, select only two of them as causes of "acquirements" in progenitors and of "innate" characters in descendants? Why ignore the evidence furnished by the evolution of the higher animals, which demonstrates that, so far from "innate" characters replacing in decendants characters acquired by ancestors, the contrary has happened on a vast scale; for, unlike low animals, the higher types (in proportion as they are high in the scale) are born helpless, and must develop mentally and physically in response to use before they can reach maturity and power of maintaining independent existence.

Dr. Bather writes with an air of surprise, "Actually we are now talking of biological method." But have we talked of anything else? What other is the insistence on precision of language and the necessity for crucial testing, and the discussion about the nature of evidence and proof? He continues, "My own difficulty has been to devise a question that should be universally accepted as crucial, or, having devised one, to elicit the relevant facts." He indicates the trouble exactly. Many biologists—among whom I am sure must be Dr. Bather himself—have tested their suppositions thoroughly and established them completely, and yet have had them rejected on irrelevant grounds by large sections of their fellows who have unlike ideas as to what constitutes evidence and what proof—Darwin with his theory of natural selection, for example. Some years ago the late Prof. William James, much amused, furnished me with a case in point. He had happened on an article which seemed to him, as it afterwards did to me, good and conclusive. This he submitted to a biological colleague, who, while disputing not a fact (the facts were indisputable) or an inference (I think the inferences were incontrovertible), quite simply declared that he hoped they had done with that sort of thing for ever. It seems he was a devotee of one of the exact and modern methods.

As to the "difficulty . . . to elicit the relevant facts," I, of course, know nothing about Dr. Bather's special troubles; but I do know that biologists in general have always extraordinarily restricted the area in which they have sought their facts, and that, in the search for exactitude and modernity, they have recently restricted it still further. Of course, other things being equal, "biologists who can experiment with their material are certainly in a far better position to perform both these operations than one who can only observe portions of extinct animals." But that is not the point in dispute. Many sciences deal with organisms that are not extinct; and while experiment may add, and, indeed, has greatly added, to our knowledge, it can do no more; for, after all, it is only one out of several ways of collecting evidence. Dr. Bather says: "Either the glaring truism is an identical proposition or it is a statement actually disputed." I believe he is not quite correct. The statement that "variation is the sole cause of non-inheritance" is, in effect, identical with the statement that, "apart from variations, like exactly begets like when parent and offspring develop under like conditions," *and* it is actually disputed. Moreover, the truism is practically identical with the statement that, "apart from variations, offspring tend to recapitulate the parental development." It is true that biologists are divided on this point—as they are divided on almost everything else—but only, I think, because they use the words "inherit" and "reproduce" as synonymous. I hope to deal with this matter shortly.

I assure Dr. Kidd (NATURE, September 1, p. 11) that there is no "catch" in this correspondence. I have used the plainest and simplest language I can think of, and have pleaded only for the use of ordinary scientific methods, including plain and precise language. I am too ignorant to reason about the hair-patterns of the modern horse; but of this I am sure, that if the collar altered the hair-pattern of the ancestral domestic horse, and if the modern colt reproduces this alteration without experience of the collar, then we have here not inheritance, but variation of a very remarkable and unusual sort. In that case the modern horse is like his domestic ancestor neither by nature nor by nurture. He is like only in very superficial seeming. If Dr. Kidd will not accept the blacksmith's arm as a good illustration, why not reason from that of the ordinary man, which also, as I say, develops in response to functional activity, and has so developed not for twenty, but for twenty thousands of generations.

G. ARCHDALL REID.

Southsea, September 6.

The Green Colouring of Surf on the Horizon.

ON December 16, 1920, at 5h. 30m. p.m. Central Java time, while the steamer *van Noort* was passing by the small island of Pisang, north of Kroë, on the western coast of Sumatra, at a distance of about 3 km., I noticed that the breakers on the shore were not white, but green. Observing the phenomenon with a binocular it was possible to see more details. The low breakers especially showed the green colour. Breakers rising higher became white, the lower right and left corners, however, remaining green. As the breaker ran forward along the shore a green corner preceded, and sometimes the separate crests seemed to be coloured. Along the shore of Sumatra, at a distance of 7 km., no particular colours could be observed. From the upper decks of the steamer the colour could be seen, but it was less intense.

The weather was calm, the sky partly clouded, and the sea grey. The horizon was strongly dipped; I saw a sharp horizon before the isle, the surf seeming to rise above this horizon. By no means could a continuous surface of the sea be seen between the ship and the island. The phenomenon disappeared at 6h. 10m. p.m., when the yellow shore-line of the isle had become invisible below this apparent horizon.

My object in directing the attention of readers of NATURE to this phenomenon is to learn whether the green colouring of surf has ever been observed before. I have been unable to find any description of it, and shall be very glad to learn whether the phenomenon has been seen and recorded by others.

As to the explanation, I believe the cause to be the same as that of the "green flash," as the phenomena agree remarkably in some respects.

Observations of the green flash have been published in NATURE (vols. 93, 94, and 95 of 1914–15). Mr. C. T. Whitmell writes:—"Under favourable conditions at sunset, as the upper segment of a yellow sun gradually diminishes, the right and left corners of the segment become green; this colour gradually spreads inwards, becoming marine, until finally the last tip of the sun may appear almost greenish-blue" (NATURE, March 11, 1915, vol. 95, p. 35).

The same observation has been made by Dr. C. Braak ("Hemel en Dampkring," August, 1915, p. 52).

Both phenomena seem to be caused simply by atmospheric dispersion of light, as is explained, for example, in a most valuable article by Dr. A. A. Rambaut ("The Green Flash on the Horizon," *Symons's Meteorological Magazine*, No. 41, 1906).

The green flash and the "green surf" are not very common phenomena. Dispersion being always present, one may wonder why the green colouring is rather rare. Evershed discussing this fact writes:—"It seems to me very probable that this phenomenon is in some way connected with the abnormal conditions which at sea produce mirage effects. The layer of dense air in contact with the sea might produce total reflection for solar rays refracted from below the horizon, but the critical angle of reflection will depend on wave-length, and it is possible under certain conditions that the green rays may be totally reflected, whilst the red are refracted" (NATURE, May 15, 1915, vol. 95, p. 286).